with administering identical or closely related branches of government; it is highly desirable although not always practicable between the residents of any district and their chosen representatives. Beyond these limits communication must be indirect—through newspapers and periodicals, industrial processes, social conventions and political machinery. Such communication can create social unity only if its processes relate themselves to a common historic background which gives authority and a measure of justification to the existing economic and political systems and at the same time suggests an ideal to guide in their reconstruction. The importance of indirect communication as a stabilizing factor in modern society deserves more recognition; it is our chief protection against excesses of local enthusiasm, against the extravagancies of closely communicating groups who are swept off their feet by new ideas or programmes engendered in the course of their own discussions.

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THE LOCUS OF TELEOLOGY IN A MECHANISTIC UNIVERSE

IT is a cause of very great regret to me that I can not meet Professor Holt on his own ground.[1] The question of the correct use of concepts which he has raised is no doubt important in itself and most interesting to the readers of this JOURNAL. But it is not for me to undertake a discussion for which I am so little skilled, since, if I am not wrong, Holt's criticism of my work can be met without passing beyond the field of science. The truth seems to be that Holt has certain very definite faults to find with the teleological conclusion which I have reached, and that he has associated these as illustrations with a larger logical criticism of the structure of knowledge. He comes at length to a result which I can only think a counsel of perfection far beyond the present powers of men of science, and also, if I have read them rightly, of most philosophers. Yet this is no affair of mine.

The two books[2] which seem to Professor Holt so full of bad reasoning describe what I believe to be a scientific fact. This description has involved not only a large amount of scientific material but also, perhaps needlessly, a small amount of philosophical argument. The philosophical argument is, however, almost though not quite entirely

[1] *Cf.* this JOURNAL, Vol. XVII., pp. 365–381.

[2] *The Fitness of the Environment,* New York, 1913; *The Order of Nature,* Cambridge, Mass., 1917.

of secondary importance. Both the scientific and the philosophical portions of the books are set forth in words—not, I will maintain, in concepts—of a certain vagueness. Holt believes that this vagueness involves real fallacies which have seriously misled me. I can but admit, with his paper before me, that it has seriously misled him. For this the fault is perhaps partly mine, but, as I hope to show, certainly partly his. Such errors are made worse, according to Holt, by one cardinal fallacy with which we may begin our examination of the question. Briefly, my critic believes that the teleological conclusion of my books is the result of a preliminary selection of three elements by me.

I

When the properties of the chemical elements are carefully studied, it is easy to see that the elements differ widely and in such a way that their exact description as a system is very difficult. Nevertheless valid approximate descriptions are not hard to find. One such description is well known as the periodic classification. Another approximate description, which puts the emphasis upon compounds rather than simple substances, is given by the statement that the properties of hydrogen, carbon and oxygen are unique. Evidently such a statement is meaningless unless the use of the word unique is defined, for of course all elements are unique. This, together with other logical considerations of the concept unique, is one of the specifications of Holt's criticism. The fact is, however, that the use of the word unique here in question is fully defined by illustrations of every sense in which it is employed, and that it is never in my writings used to imply anything but its clearly stated content. For example, the heat of formation of water is the highest heat of formation of any compound from the elements, the solubility of carbon dioxide is such that it distributes itself equally between a liquid water phase and a gas phase, the heat of vaporization of water is the greatest known heat of vaporization, the number and variety of compounds of carbon, hydrogen and oxygen is the greatest among all known substances, oxygen is chemically the most active element, solutions containing carbonic acid and bicarbonates are (other things being equal) the weakest in acidity or alkalinity of any known solutions. In general the properties of these three elements and of their compounds very often fall at singular points (maxima, minima, points of inflection, *etc.*) on the curves representing the properties of all known substances. Thus it is a significant and useful approximation to a description of all the elements to say that the properties of these three are unique, and this statement ought not to lead to any misunderstanding.

Holt, however, declares that I have *selected* these three elements for study and that it is because I have selected them that I find them teleological. For the moment it will suffice to deny this criticism and to insist that my statement is a valid approximation taking account of all elements. Of course I shall raise no objection to the use of any other word rather than unique as a designation of the various singularities whose methodical enumeration is the definition of the sense of the word in my writings.

Thus the first point that I have to make is that the system of all the elements may be correctly characterized by the statement that the properties of hydrogen, carbon and oxygen are unique, when it is understood that the word unique is used to designate such peculiarities as are above mentioned and more fully described in *The Fitness of the Environment* and in *The Order of Nature.*

In studying this question an effort was made to consider all known properties of matter and, though complete success is manifestly impossible, I believe that the analysis has been adequate. Unfortunately this is a question for the specialist, who must be referred to the full discussion.

Professor Holt's argument that teleology appears in my writings because at the outset three elements were selected for consideration seems therefore to be due solely to a misinterpretation of the scientific evidence. In order to justify it he will have to show that this is erroneous or inadequate.

A second point which must now be taken into consideration, but which Holt seems to find unobjectionable, is that Willard Gibbs's characterization of physico-chemical systems is a sufficiently complete and exact description of the conditions of all physico-chemical events, regardless of the peculiarities of such events and especially of the chemical elements which are concerned in them. In other words the variables of Gibbs's mathematical analysis—phases, components, temperature, pressure, concentrations, *etc.*—are the necessary and sufficient variables for the exact characterization of any physico-chemical system, absolutely without regard to the specific properties of whatever substances may make up the system. Of course the particular values of the variables in any case will depend upon the specific properties.

A third and final point remains to be considered. The distribution of properties of the elements, which is approximately summed up in the statement that the properties of hydrogen, carbon and oxygen are unique, constitutes an unmistakable pattern. When this pattern is examined in the light of Gibbs's conclusions it is apparent that almost any sensible change in the pattern affecting almost any

one of the properties of the three elements hydrogen, carbon and oxygen would involve a great restriction upon the possibility of physico-chemical events, because systems must then be less numerous, less varied, or less stable. This is due, first, to the fact that the unique properties of the three elements are often uniquely favorable. Thus the great number of compounds favors number and variety of systems and the chemical activity of oxygen favors activity. Secondly, it is also due to the fact that the properties are important not merely individually, but also jointly in groups. Thus the solubility and aeidity of carbon dioxide stabilize chemical conditions in water phases, and the various thermal properties of water coöperate to stabilize physical conditions generally.

Holt, overlooking such instances of coöperation of several factors, in spite of their frequency in my writings, makes much of the fact that when a phenomenon depends on such coöperation of several factors it is by no means true that a singular or unique value in a given factor is necessarily most favorable in the result. Yet he has but to look into my books in order to find examples of this, such as both the physical and the chemical relations between water and carbon dioxide or the chemical relations between carbon, hydrogen and oxygen.

But, indeed, my whole description of the relation of the properties of the three elements to the characteristics of systems is an illustration of the coöperation of factors in a manner so intricate and so varied, involving not merely individual properties that are maxima or minima, but also combinations of properties not themselves maxima or minima, yet nevertheless so related that maxima and minima result, and then combinations of these combinations, that it is a cause of amazement to me that Holt should ever have ventured such a criticism at all. His conclusions seem to me to depend upon a further misconception of the scientific part of my writings.

At all events, every consideration to which Holt refers has been in my mind while discussing the relevant questions, and finds expression at many points in the two books. I am unable to justify this statement except by the reference, for nothing less than the whole scientific analysis of the relation of the properties of the elements to the conditions of physico-chemical activity, stability and diversity is in question. Perhaps, however, an answer to one of Holt's specific criticisms may further illustrate the situation. My critic points out that while I have emphasized the high surface tension of water, that of mercury, as I did not fail to specify, is much higher. This is an example, however, of the phenomena important in systems depending not upon one, but upon two properties in coöperation. Capillary phenomena, in fact, depend upon the ratio of surface tension to

density, and other types of surface phenomena upon surface tension and solubility jointly. Thus what Holt puts down as a defect in the catalogue of properties of water is in reality a simple instance of a relation which elsewhere he accuses me of quite overlooking.

It may appear to the reader that something nevertheless remains of the fact, taken by itself, that the surface tension of water is not a maximum, and it is indeed true that nothing would be easier than consciously or unconsciously to juggle this kind of an argument. In this place I can only say that I have always been aware of this danger and have done my best to point out all qualifications of the uniqueness of a property or of the relation of a property to the characteristics of physico-chemical systems. Such errors of this kind as may exist in my books have at least not been revealed by Holt's criticisms.

The important fact; and the principal scientific conclusion of the two books is that if any one of a large number of properties of carbon, hydrogen and oxygen were not what it is, but resembled that of any other element, the whole time process would be reduced to almost nothing. Without the chemical combining power of carbon for hydrogen, the chemical activity of oxygen, the solvent power of water, or a high heat of formation of water—in case a single one of these or a single one of many other properties or relations between properties were not approximately what it is—the whole evolutionary process would be greatly restricted. What is the explanation of the fact that the properties possessed by these three elements are thus related? At present this question can not be answered; but the facts which suggest it are beyond the reach of the kind of criticism that Professor Holt has written, for they do not depend upon my lack of skill in the use of concepts or of words.

II

In his famous work *Exposition du Système du Monde* Laplace calculated the probability that, as a chance occurrence, the planes of the orbits of all the members of the solar system should be as nearly coincident as they are, and that all the planets and their satellites (so far as they were then known) should rotate and revolve in the same direction and approximately in the same plane. His calculation, hardly indispensable in view of the obvious magnitude of the improbability, led him to the conclusion that there must be some explanation of coincidences so improbable as chance occurrences.

Such a conclusion is entirely acceptable to the man of science. Indeed, one of the most familiar characteristics of scientific method is to seek an explanation or cause for any group of coincidences, or for any pattern, which, regarded as a chance occurrence, is sufficiently

improbable, provided the pattern is unmistakable. Thus the pattern in the solar system discovered by Kepler, and described in his three laws, led to Newton's *Principia;* thus coincidences in the geographical distribution of species to *The Origin of Species,* and Boyle's discovery that the values of $P \times V$ for a gas are coincident to the kinetic theory of gases and of heat.

It is well known that the logical implication of the mathematical theory of probability and even of Darwin's argument concerning the flora and fauna of the Galapagos Islands has not been generally agreed upon. But in spite of the familiar contention that it is idle or meaningless to calculate or to discuss the probability of that which exists, and the equally familiar argument that, according to the postulates of the theory of probability, any sequences of events or any temporal or spatial order is as probable as any other, this habit of the scientist is universal and successful.

Therefore I must once more decline to meet an argument of Professor Holt's. It is certain that I should never succeed in stating this argument from probability so as to satisfy him. And that I am under no obligation to do so seems no less evident. For me probability is still, as Laplace once said, *"le bon sens reduit au calcul."* Let there be no misunderstanding on one point, however: it is not to the authority of Laplace or of any one else that I appeal, but to experience.

Accordingly the statement may now be made that the relation between the properties of hydrogen, carbon and oxygen and the characteristics of physico-chemical systems is not due to chance, but that there must be a relevant explanation or cause. Just as Laplace calculated the improbability of a certain set of coincidences in the solar system, so the improbability of this set of coincidences might be calculated. But, since the magnitude of the improbability is obvious, this is unnecessary.

At present it seems safe to say that the properties of the elements and the characteristics of systems, like the properties of triangles, are changeless in time. Recent evidence from the study of radium and of electrons, while indicating slight variations in what we call atomic weights, in other respects strongly supports this view. And so far as the atomic weights are concerned what is involved seems to be merely a slight change in a single definition which itself does but strengthen the conclusion.

This is why I have spoken of the *connection* between the properties of the three elements and the characteristics of systems as teleological. If changeless in time it must be in a justifiable sense of the term an absolute property of the universe, and can not be the

result of the only process by which so-called adaptations arise—the time process. It is therefore at present inexplicable, but surely not quite indescribable. For example, it bears some slight resemblance to the internal arrangement of a watch, if I may choose a time dishonored example.

And thus we arrive at the point where Holt and I are in complete agreement. To proceed from this conclusion with the old natural theology, and speak of design and mind behind the teleology, or even to suggest, or however indirectly to imply, design, seems to me, as it does *a fortiori* to Holt, just such a voluntary and irresponsible act as he thinks that I have committed at an early point in my argument.

Here once more I should be quite willing to change the term for another not more misleading than teleology. But the word teleology has always been used in this sense, as well as in several others, and I have found no other word.

The conclusion is this, that teleological pattern has not merely originated out of chaos by adaptation, but that there has always been an underlying teleological order of nature.

III

If it were true that my writings contain some of the statements which Professor Holt attributes to them, much of his criticism would remain valid. In particular, his defense of the mechanistic theory, which must seem well founded to most men of science, would cause me great inconvenience. For here I believe him to be right. But this defense is the result of his assertion that I have declared that certain empirical data "argue a relation between past phenomena and present that is not mechanical." This statement is an error. I am aware of no such data and can not imagine such an argument. So far as known the physico-chemical universe like the mechanical watch runs mechanistically.

Probably Holt and I agree about what we both clearly understand. But the logical and epistemological concepts which he handles with so much skill are beyond my powers, and the tissue of absurdity which he so readily destroys was not woven by me, nor is it the locus of teleology.

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